A Formalist's Reading of Some Functionalist Work in Syntax

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Abstract

In terms of social groups, "formalist" syntacticians are roughly those who follow some variant of the program of Generative Grammar (Principles & Parameters, LFG, HPSG, Minimalism, etc.). Their analyses tend to be based on assumptions of Modularity (including the "Autonomy of Syntax" as a special case) and categoriality, among other principles. In more general terms, however, it can be argued that "formalism" simply consists in a commitment to fully explicit formulations cashing out one's intuitions about the structure of language in terms that require as little as possible in the way of unanalyzed contributions by an understanding reader: surely a 'motherhood' issue that could not plausibly differentiate theoretical views.

"Functionalists," typically, are those who argue for a higher degree of involvement of other domains (semantics, pragmatics, discourse, extra-linguistic exigencies deriving from the context of communication, etc.) in syntactic phenomena, and for hierarchies, gradients, and other non-categorial analyses. I argue, however, that the practice of many functionalist syntacticians generally trades heavily on a relatively low degree of explicitness and on pre-systematic, intuitive understandings of the categories of an analysis. When functionalist arguments against modularity, or in favor of hierarchical scales as opposed to discrete categories are examined closely, they often break down on just the basis that they involve assumptions about the unity of domains of fact that are better seen as the product of distinct interacting systems. The activity of examining functionalist arguments in this way is often quite instructive, but not always in the direction their formulators might have intended.

My (assigned) topic here is the question of what formalists can learn from functionalists in syntax. "What can we learn…" might be interpreted as: what analyses have we seen that gave us ideas? Naturally, when one reads the work of any other linguist who cares about the linguistic material under discussion, there are likely to be descriptive points that will be of interest, but no particular broader purpose would be served by an enumeration of cases in which I personally have found examples for my own arguments in functionalist sources.

Another way of looking at the question would be to ask: what points have functionalists made that would cause me to abandon a formalist program? I'm afraid that here I have to report that nothing I have read in the functionalist literature has convinced me of any such thing. Certainly there are phenomena there that still want explanations, but nothing suggests to me that these by themselves warrant basic revisions in methodology, as opposed to the various revisions of particular analyses that ought to be adopted in specific cases.

While the way I will address the question in this paper will inevitably have certain autobiographical aspects, I will attempt to formulate the issue in somewhat more general terms. When I read the work of a careful functionalist, how would I address the points made? That is, where functionalists cite empirical data, especially data that are intended to challenge the validity of other points of view, I am interested in the extent to which those data actually bear on issues in the theory of formal grammar, and where they do, how. In cases where the bearing on issues of principle is less than what is claimed, one also wants to know whether this results simply from poorly constructed arguments, or whether it reflects some broader principle that characterizes an entire approach to language. In the process of considering those questions, I think I can learn something about what formalism is about, about ways in which the functionalist program shows less than it purports to, and about some broad methodological differences in linguists' approaches to the subject matter of our discipline.

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1 What is a Formalist?

In terms of the sociology of the field, I imagine some of my "formalist" friends would consider me a somewhat marginal member of their fraternity, and the first point I want to make may solidify that impression. In particular, I have rather serious doubts about the ultimately productive nature of some important assumptions in recent formal syntax, and about a style of argument that gives rise to them. Some "formalist" work seems to me to be driven by just exactly the wrong sense of "formalism": that is, formalism for its own sake, an approach to the field that allows linguistic research to be driven by the æsthetics of a notation. It is one thing to let the consequences of one's formalization suggest hypotheses for exploration — it is quite another to act as though those hypotheses were themselves empirical results.

To cite an example where my opinion will offend a good number of syntacticians whose work I otherwise admire, consider the foundation for the wholesale replacement some years ago of the traditional notation for clausal structure in terms of the categories S and $\overline{\text{S}}$ by a notation in terms of abstract functional heads. Originally, this consisted in the replacement of S by (some projection of) INFL; subsequently, extending the same line of argument at the instigation of Pollock (1989), INFL was itself superseded by a host of AGR's, T's, ASP's, etc. by way of the "exploded Infl" hypothesis. To call this a "hypothesis" has rapidly become a misnomer: it is, rather, the basic working assumption with which beginning students are provided for the discussion of syntactic structure. The result of this line of thought is a climate in which the burning questions for formalist syntacticians have come to be ones like "Is AGR_sP above TP or below it?" rather than "Is subject agreement a syntactically autonomous constituent of representations even though it forms part of a single word with the main Verb?"

When we ask what the basis was for the whole cottage industry of functional heads in clausal structure, we can, I think, trace it back to Chomsky's discussion on p. 3 of *Barriers* (1986), which I cite in its entirety:

> Does this system [$\overline{\text{X}}$-theory as developed for the primary lexical categories — sra] extend to the nonlexical categories as well? Evidently, the optimal hypothesis is that it does. Let us assume this to be correct. Then the clausal categories conventionally labeled S and S' might be I'' and C'', respectively, where I = INFL and C = complementizer.

This strikes me, as it also struck Lightfoot (1990), as a rather stunning logical

leap into the abyss, but it quickly led to a proliferation of structural articulation and a reformulation of the nature of syntactic structure for which the empirical foundation always seemed rather weak to a disinterested bystander. In fact, in Chomsky's own writing one finds numerous arguments in this same style, places where a decision is made to pursue a certain set of assumptions because that is what is suggested by the properties of the formalism of the moment, rather than because of a strong intuition that the empirical grounding of these particular assumptions is strong. Sometimes following this path leads to quite genuine insights, but at other times considerable distraction and detour has resulted. In the case of the 'exploded INFL hypothesis,' it is only quite recently, with the Minimalist program (Chomsky 1995 and elsewhere), work of Edwin Williams (1994), my own work on inflectional morphosyntax (Anderson 1992), and others, that the AGR's have begun to atrophy and the depth of complexity of posited functional structure has begun to be reduced again, though in at least some cases, I fear this is simply because the æsthetics have changed, not because the issues have been rethought on genuinely empirical grounds.

As is presumably evident from the tone of these remarks, I do not believe that formalism of this extremely 'pure' sort, insulated from grounding in mundanely empirical considerations, is to be encouraged. Of course, if a formalism is really serving its purpose, it *should* suggest lines of inquiry to pursue, but such suggestions should not be confused with marching orders. Whatever the similarities between linguistics and mathematics, ours is not a science in which creative elegance alone constitutes a significant result. Of course, letting the properties of a formalism suggest items for a research agenda has often proven to be a productive strategy, and I certainly would not claim that the consequences of pursuing the study of functional categories in the way that grew out of Chomsky's remark above has been a waste of time. But let us keep our priorities straight: the aim of linguistics is insight into the nature of language, not elegance for its own sake.

Rather than confusing formal elegance in itself with empirical results, an alternative conception of the rôle of formalism in Linguistics is to see it as simply a commitment to explicitness, a way of fully explicating the structure we believe we find in language. As a formulation of what I am getting at here, I was quite taken with some remarks I heard recently from Jacob Lurie, the high school student who won first prize in the 1996 Westinghouse Science Fair. The young man was being interviewed by a reporter from NPR; since the prize was for his work on the computational properties of "surreal numbers," the interviewer tried to get him to talk about just what those were for a while, until it became clear that the answers were not turning into great radio, and he then shifted the topic to the more general

question of just what kind of activity mathematics is. I do not have a transcript of exactly what Mr. Lurie said, but it was roughly the following:

> "What do you do when you do mathematics? You think about some kind of object, and you develop your intuitions about the object. Then you try to express those intuitions in terms of a formal system. Then you explore the properties of that system, to see if they really do correspond to the intuitions you had about the kind of object you're trying to understand."

The point here is just the one that ought to be made about formalism in Linguistics: the goal of a formalization is not elegance in itself, but rather assistance in cashing out one's intuitions explicitly. What may have a chance to count as insights and results are really these intuitions: our understanding of what it is that is systematic and coherent in the workings of language. As scientists, we want to explicate these intuitions as fully as possible, with nothing left to the imagination or creative understanding of the reader. In this sense, a commitment to formalization would seem to be a 'motherhood' issue. Everyone has to want to be explicit — that's just part of what makes what we're doing "science."

Or so you'd think. As Croft (1991:275) observes, "some functionalists (e.g., George Lakoff) [. . .] argue that their theories should not and even cannot be so formalized." Like Croft, I will pass over this position without much comment. But I do think that a great deal of work within the functionalist approach, or at least some important exemplars of that approach, trades in essential ways on inadequate degrees of formalization in this generic sense of explicitness. In particular, the notion that descriptive categories, and especially those of traditional grammatical description, are somehow given, so as to define the object of inquiry in the study of language, rather than themselves requiring formal explication and reconstruction, seems to me to underlie some poorly founded functionalist criticisms of the activities of targeted formalists. I want to stress that this particular problem does not seem to me to be inherent in the foundations of the functionalist agenda. It is more of a lifestyle than a "virtual conceptual necessity," but it does appear to be endemic in certain circles.

2 How have Functionalists influenced Formalists?

Functionalism itself, as an approach to grammar, means rather different things to different people. Croft (1995a) gives a survey of diverse schools of functionalism

that seems generally fair to me. In his terms (Croft 1995a, p. 490), the basic point is that "[f]unctional analyses of grammar [. . .] center on linguistic explanation based on language's function in a larger context." That is, they argue that the nature of what language **is** follows from what speakers **do** with it. Differences among functionalists then tend to center on the extent to which they insist that is not even possible to formulate the structural properties of language without essential reference to matters that are outside of the system of language itself.

To designate two more or less opposing communities of syntacticians, then, if the "formalists" are those who are committed to notions involving the autonomy of the syntactic system, or the appropriateness of a modular analysis which abstracts various parts of the system from others and analyzes them in self-contained ways as individual contributors to a more complex overall reality, the "functionalists" are those who would deny the basic adequacy of such a point of view.

At one end of Croft's spectrum of functionalists are people like Susumu Kuno, Ellen Prince, and my colleague Larry Horn, who do things rather similar to what formalists do, with the difference that their analyses often make explicit reference to meaning, pragmatics, and discourse function. Work of this sort, it seems to me, challenges not so much the basic notion of modularity in grammar as the actual boundaries of the internally coherent modules. Perhaps that is why it does not figure prominently in the polemics of either side.

On the other hand, perhaps precisely because of the similarity in assumptions and styles of argument, it is fairly easy for non-functionalists to learn from a close examination of this kind of work. For example, a series of works by Kuno (1972, 1987, Kuno & Kaburaki 1977) dating back some years made it clear (to those who listened) that a unitary, purely structural condition (such as Principle A of the binding theory) could not be developed to account for all instances of reflexive pronouns even in English, let alone in all natural languages. Kuno argued from this that notions such as the speaker's empathy with particular participants and other ideas with a strong functionalist flavor were crucially implicated in a comprehensive theory of reflexives.

Other writers have taken this point quite seriously; but rather than concluding that an account of reflexives lies outside of grammar, they have responded to it by developing a more finely articulated theory. Building on considerable previous work (much of it summarized in Koster & Reuland 1991), a view that is often identified particularly with the work of Reinhart and Reuland (1991, 1993) distinguishes "local anaphora" from "non-local" or "logophoric" anaphora. The former represents the class of reflexives that fall under a structural condition like Principle A; non-local reflexives, on the other hand, represent a quite distinct cat-

egory, with quite distinct logical and grammatical properties for which a rather different account is developed. The distinctness of these two categories can be shown not only from language-internal considerations, but also (as my colleague Sergey Avrutin has argued) from the empirical data of child language acquisition and aphasic impairment. The result of keeping them distinct is a much sharper and more nearly adequate formal theory of anaphoric binding, together with at least the outlines of quite a different area of grammar.

Notice that the point of this analysis is neither the denial by "formalists" that the points made by a functionalist analysis like Kuno's are interesting or relevant, nor the rejection of the idea that a structural principle of anaphoric binding is one component of an autonomous, modular syntactic theory that governs important regularities of the structure of language. Rather, what (formalist) syntacticians such as Reinhart and Reuland see in these observations is evidence that the apparently unitary category of "reflexives" is actually internally diverse, with some cases falling under one sub-theory of grammar (the theory of binding, construed fairly narrowly as a set of purely structural principles operating over phrase markers) and other cases falling under another (the theory of non-local binding, or logophoric reference). It is precisely because the overall category of "reflexives" involves the interaction of principles from (at least) two quite distinct domains of grammar that a unified account seems elusive, as Fasold (1996) argues; but the right response to that state of affairs is not to reject "formalism," but rather to work at teasing apart the factors whose joint contributions are evident in the complexity of the surface facts. We will see that this case is not at all atypical.

Within this same general area of grammar, the theory of reference, we can note another interesting influence of functionalist work on theories of a clearly formalist character. Surely one of the areas in which functionalist views have been most extensively developed, continuing from the studies of the Prague School and earlier up through the present day, is the grammatical organization of sentences to reflect new *vs.* old information and related distinctions. While this literature has generally relied on unformalized intuitions about the information structure of sentences and of the discourses in which they occur, not even the most rabid formalist would deny that there is much of great value and insight to be found there.

Quite explicitly building on this background, the related positions of Discourse Representation Theory (Kamp & Reyle 1993, Kamp 1981) and File Change Semantics (Heim 1989), and more broadly the "Dynamic Semantics" movement have made considerable progress in formalizing many of the relations between discourse and reference brought out by functionalist accounts. In fact, I am pretty

sure that the theory of reference that emerges from these considerations is just what is needed to under-pin the theory of non-local or logophoric anaphora referred to just above, but I will not develop that claim here.

I do want to note, however, that further extension of the notions of File Change Semantics to encompass even broader areas of the informational structure of sentences in a formal theory are currently quite an active area of research, as illustrated by the work of Vallduví (1992). The productivity of this particular formal explication of core functionalist insights, when taken together with a syntactic analysis along standard lines, will be evident in the analysis of Japanese post-verbal constructions presented by Kaiser (1996).

In short, then, some functionalist work is in fact close enough to that of your canonical formalist to make dialog and reciprocal interaction fairly straightforward, and some areas that have been primarily cultivated by functionalists may well be quite suitable for formalization. Let us remember that this **is** a two way street: after all, Kuno was led to his observations about "speaker empathy" in non-local binding cases by noting apparent inadequacies in existing formal theories of the grammar of reflexives. Other functionalist work, such as much of the literature on the information structure of sentences, implies (though it does not provide) a kind of explication that can often be pursued in the development of formal theories, typically with some profit in the form of a better developed sense of the internal structure of the domain of phenomena involved.

At the other extreme of functionalist views we find the position alluded to above which maintains that serious analyses of language cannot in principle be formalized, or that of linguists who argue that **all** properties of grammatical structure reduce to matters of understanding, pragmatics, communication, etc. — that is, that there is no distinct realm of grammar at all. This view seems to fly in the face of the fact that the study of grammar has apparently made quite considerable progress by assuming that there is something there to study. I cannot really claim to understand this position, though, or why anyone would actually want things to work out that way, and so I will not consider it further.

3 Typology and the Functionalist Agenda

The remainder of this paper will primarily concern the variety of functionalism associated with the study of typology, especially as exemplified in the work of William Croft (cf. Croft 1990, 1991, 1995b). Among typologists, Croft has been rather more explicit than most about just how he sees the differences among for-

mal and functional approaches to language, and his interests come quite close to ones I have been concerned with myself at various times.

"Functional/Typological" syntax has often been presented as the alternative to "Formal" syntax, especially with respect to the discovery and characterization of universals of language. This tradition sees itself as originating in the work of Greenberg (1963). Despite the sense among functional typologists that they constitute the more or less official opposition in a discussion often dominated by formalists, there has actually been very little real debate. As noted by Matthews 1993, "for most of the past fifteen years, despite occasional disparagement from one side or another, each school has in practice had little reason to refer to the other. It is worth noting, for example, that [Croft 1990] cites no work by Chomsky. Seven years before, Newmeyer, in a book on *Grammatical Theory* whose references are wide ranging [. . .] mentioned Greenberg and his school once." [p. 45]

In general, advocates of each approach to language universals have tended to disparage the the other. Functional typologists commonly adopt an attitude (which I personally find completely unwarranted) to the effect that formalist (or to personalize the matter, "Chomskyan") linguists work only on English and thus ignore the evidence of a wide variety of languages. Sociologically characterized formalists, on the other hand, claim that the analyses of typologists are so completely superficial as not really to be worthy of consideration in evaluating hypotheses about grammar. Recently, it should be noted, we have had some more illuminating exchanges, such as as that between Newmeyer 1992 and Croft 1995a, as well as the Milwaukee symposium at which the present paper was presented, where something more in the way of mutual understanding has been sought. It still remains to be seen how much of that there is to be found, though. Pious and conciliatory intentions do not constitute results in themselves.

How much difference is there really between typology and formal grammar? In this connection, it is illuminating to look at a recent volume surveying *Approaches to Language Typology* (Shibatani & Bynon 1995). In addition to Croft's Greenbergian view (entitled somewhat eclipsingly "Modern Syntactic Typology") and a paper by Greenberg himself on the relation between typology and change, we find presentations of the work of the Prague School, of groups in Paris, St. Petersburg and Cologne whose individual approaches I will not attempt to characterize here but which are each distinct in various ways from that presented by Croft and Greenberg.

The last paper in the volume, however, Fukui 1995, approaches the typological characterization of a language from the point of view of straightforward formalist

("Principles and Parameters") theory. On this picture, the syntactic system of a language, or at least important central aspects of that system, are to be specified in terms of values for the parameters provided by Universal Grammar that define a limited range of possible variation in grammars. Rhetoric aside, it is hard to see how this project differs from the basic goals of the other conceptions of typology represented in this volume, except, perhaps, in the explicitness of its formulations. Exploring the dimensions of typological variation is not different in principle from exploring the parameters of variation in grammar. On the other hand, developing a theory of Universal Grammar does not make sense without consideration of a range of languages, a point which is quite clear to Fukui and other adherents of the Principles and Parameters program.

If there is a difference between formalist and functionalist work in typology, then, it would seem that the distinctions are to be sought not so much in the basic questions about the subject matter as in the range of answers that are proposed. And in fact, there are clear differences in what counts as an analysis in the two literatures. As Croft explains in some detail in his introductory text (Croft 1990), the functional-typological approach seeks to find (implicational) hierarchies in the data of inter- and intra-linguistic variation, and characterizes basic categories and terms of linguistic analyses by scales, gradients, and fuzzy-edged prototypes. Formalist accounts, in contrast, seek discrete, categorial analyses and fully explicated distinctions. Rich deductive structure is sought not so much as an end in itself as a means to reducing the primitives of grammatical variation to a logical minimum of genuinely orthogonal dimensions of variation among languages.

In the nature of things, since they seek to find grounding for grammatical phenomena in "language's function in a larger context," functionalists *expect* to find substantial inter-penetration of phenomena. The holistic view that every aspect of language is permeated by such factors of meaning and use is presented not as an empirical hypothesis, but rather a basic research strategy. Whenever these seem relevant to a full account of linguistic behavior, functionalists find confirmation of their view that language is not to be explained by autonomous, self-contained modules.

Formalists, in contrast, respond to such observations by proposing a division of labor, with various parts of the explanation provided by distinguishable components of linguistic knowledge whose working can be described independently of one another, and whose interaction gives rise to the full complexity of the phenomena. This results in a high degree of modularity, which in turn supports the autonomous formulation of various parts of grammar.

What is at stake here is not just a difference of intellectual style. I think it

actually reflects a rather fundamental difference between two notions of what linguistics is about. The confirmation functionalists tend to find for the necessity of continuous, scalar, prototype-based accounts is related to the fact that the object of inquiry in (much) functionalist work is really rather different from that in (much) formalist research. If what the linguist is interested in is the *capacity* of the human language faculty, as most current schools of formalist syntax would maintain, then data about usage and frequency — or even the issue of whether a given possibility is ever actually instantiated in any language — may be of at most rather marginal interest. On the other hand, if (along with much of the functionalist community) what you seek is a unified account of the *activity* of human language users — what speakers **do** with language — then all of these domains that go beyond just the consideration of what the human language faculty **is** seem crucial.

I have argued elsewhere (Anderson 1992, chap. 13) that the scope of the language faculty cannot be derived even from an exhaustive enumeration of the properties of existing languages, because those contingent facts result from the *interaction* of the language faculty with a variety of other factors, including the mechanisms of historical change. To see that what is natural cannot be limited to what occurs in nature, consider the range of systems we find in spontaneously developed language games, as surveyed by Bagemihl (1988). These obviously include many sorts of transpositions and manipulations of phonological form that correspond to no occurring phonological or morphological rule of any language, but which seem nonetheless to fall within the capacities of language users, once developed for the special purposes such systems serve.

I would argue that the reason for the non-occurrence of many of these processes in natural languages is simply that there is no plausible path of linguistic development which, starting from any attested system and proceeding by the possible mechanisms of phonetic, phonological and morphological change, could give rise to them. It is precisely when the content of the system is divorced from such natural development that its full potential range can be explored, and when that happens, we see reason to believe that the underlying faculty is rather richer than we might have imagined even on the basis of the most comprehensive survey of actual, observable languages. This does not mean that empirical observation is irrelevant to an understanding of the language faculty, but it does mean that it is not sufficient, and in fact observations about preferences, tendencies, and which of a range of structural possibilities speakers will tend to use in a given situation *are* largely irrelevant to an understanding of what those possibilities are.

On the other hand, if you want to capture the full reality of linguistic activity (as opposed to capacity), then issues of attestation, frequency, tendencies, marked-

ness, etc. come into play more centrally. These less categorial effects result from the complex interaction of linguistic capacity with such additional factors as accidents of occurrence, paths of possible historical change, various external, cultural, and situational determinants of use, etc. Of course, it remains to be demonstrated that this broad research agenda can lead to genuinely illuminating science.

It seems to me that in its attempt to treat "Language" in the most comprehensive sense as a unitary object of study, the functionalist view systematically confounds these effects in a way that results in a considerable amount of intellectual confusion. It is far from obvious that the intersection of all these factors actually constitutes a coherent object of study in its own right. In the natural world, for example, the wind is certainly a phenomenon we can identify, but would it make sense to try to develop a unitary science of wind? Surely "wind" results from the interaction of a wide range of climatic, geographic, atmospheric and other factors, and the way to understand it is as the product of their interaction — not as a unitary object of study on its own. Similarly, I see no reason to abandon the assumption that an extensively modular approach is the best way to attack the problem of finding the order and coherence in language through scientific inquiry.

4 A Style of Functionalist Argument

Some of the issues separating formalist and functionalist methodologies can be illustrated in relation to the following basic principle of epistemology, originally attributed to Walter Reuther:

(1) "If it looks like a duck, walks like a duck, quacks like a duck, then it **is** a duck."

We might approach this as a strategy for the scientific investigation of some important concept in grammar. Indeed, it is fairly routine to develop some series of "tests" to which we can subject linguistic objects to determine their status with respect to significant categories: for instance, to determine whether or not something is "really" an object in a given sentence, we might ask whether it is marked with Accusative case, can become the subject of a corresponding passive, etc.

Croft 1991 develops this sort of strategy in some detail in connection with basic issues such as membership in major lexical categories, the grammatical relations borne by arguments within a clause, and others. His strategy is to identify as many properties as possible that are associated with the category in question,

and translate each of these into such a test. The range of such "behavioral tests" that can be brought to bear on a particular issue can get quite large, as in the case of the properties of subjects explored in Keenan 1976 (a work which can be seen as the ultimate embodiment of this approach).

As only a slight caricature, I think that if Croft were to apply principle (1) above in this way, he would discover (after a serious consideration of known facts concerning ducks) that there are hardly any true, full-fledged ducks. Some potential ducks pass more of the tests than others — some few may even pass all of the tests — but most fail one or more of the tests that we could devise for duck-hood. His response would probably be to suggest that the notion of a "duck" is actually a kind of prototype, to which some objects approximate more or less well. The notion is a bit fuzzy around the edges, with some central exemplars and a number of near misses as well, of course, as a great many clear non-ducks.

Formalists are generally not content to settle for prototype ducks, near ducks, etc. By and large, they want the world to be organized into the ducks and the non-ducks, period. In my own contribution (Anderson 1976) to the volume (Li 1976) in which Keenan's paper appeared, I tried to deal with the fact that supposed properties of subjects, when translated into tests and applied to ergative languages (to which a number of other papers in the present volumes are specifically devoted), turn up a surprisingly robust set of somewhat-but-not-really-quite ducks. My strategy was basically to take some potential phenomena as criterial, while relegating others (particularly matters of overt morphological form) to a separate, non-criterial status. Croft (1991) takes me to task for making such an apparently arbitrary selection, apparently motivated only by my desire to come up with a completely consistent way to tell the ducks from the non-ducks.

If that had indeed been all that was at work in my analysis, Croft's criticism would have been well warranted. Actually, though, there was a bit more going on. Indeed, the construction and selection of some particular set of tests for a category, while quite a standard activity in the field, is pointless until some theory is presented in terms of which the criterial nature of the tests makes sense and finds its basis.

Instead of considering principle (1) above as synthetic, as an empirical strategy for picking out ducks in nature, let us take the alternative view that it is analytic: that is, a <u>Theory</u> of ducks would be some sort of system of propositions within which various properties and entities (including ducks) appear. Insofar as the system correctly reconstructs our intuitions about ducks, we can say that it <u>defines</u> a duck as something that displays these properties and satisfies these propositions. In other words, instead of assuming that we know all about ducks in advance, and

are trying to give an effective field guide, let us suppose that what we are trying to do is to reconstruct the content of being a duck in scientific terms. Instead of being content with the observation that some things are more duck-like than others, we would like to go on to develop a more explicit account of the potentially independent dimensions of duckiness.

That was what I was trying to do in the paper referred to above, along lines that are fairly standard in the formalist literature. I started from the premise that the notion of "subject" in pre-systematic discussion was potentially a heterogeneous one. From the point of view of the syntax, it seemed plausible that the notion of being a subject could be defined in terms of position within a phrase marker, on the premise that the structure and inter-relations of phrase markers are the essence of syntax. Again from the point of view of the syntax, structure-sensitive operations that map phrase markers into other phrase markers ought to treat elements in a consistent way on the basis of their position in a phrase marker alone. To identify a notion of "subject" within the syntax, then, what one wants to do is to find a set of such operations, and treat them as criterial for the syntactic notion in question. In terms of then-current theory, tests that seemed to correspond to this criterion included the location of PRO in infinitival constructions (in the vocabulary of the time, the target of "EQUI-NP deletion"), the binding of reflexives, and the notion of what positions count as "parallel" in the formation of coordinate constructions.

When I applied these tests to a variety of "ergative" languages, I found that they yielded results that were as consistent in those languages as they were in accusative languages with respect to when an NP constitutes a subject; and furthermore, the same NP's turn out to be the subjects, by and large, in sentences that are translation equivalents between the two sorts of languages. I took this to confirm the notion that there is a consistent notion of subject, internal to the syntax, which is remarkably invariant across languages.

Now of course, being a syntactic subject is only part of the massively complex notion of "subject" that has developed in talk about language over the past couple of thousand years, and it was actually this complex and heterogeneous nature that was Keenan's major point. The very existence of ergative languages makes it clear that the syntactic subject will often fail to meet our morphological expectations about the relation of the subject to the forms of words (that subjects should appear consistently in the nominative case, the verb should agree consistently or exclusively with them, etc.). Indeed, the facts of Dyirbal make it clear that the syntactic subject will not always meet our expectations about the semantics of subjects (that they should be agents where possible, etc,) either.

But to identify a syntactic notion of subject is not at all to denigrate these other notions, or relegate them to second-class status: it just means that we have to explicate other dimensions of the complex concept "subject" in other ways and within other sub-theories of grammar, such as the theories of how syntactic organization is related on one hand to semantic argument structure, and on another to the categories of overt morphology. If some of these other theories are unusually simple and consistent, it might turn out that syntactic subject-hood would be a consistent predictor of everything else, but of course Keenan has already shown that things are not that simple.

A particularly strong tendency that has a lot of tradition behind it is to assume that the theory that relates syntactic structure and morphological class will turn out to be especially simple, such that the inflectional categories of word form will turn out to be essentially isomorphic with the categories of the configurational syntax. Perhaps that expectation arose from the fact that for much of the history of grammatical discussion, there has not really *been* any distinct theory of configurational syntax, and what went by the name of syntax was mostly just "applied morphology." But we have not been in that state for the past forty years or so, and a serious look at the relation between morphological and syntactic form turns up considerably more arbitrariness and complexity than we are used to expecting, as I tried to show a few years ago in Anderson 1991. To the extent that is true, the interactions of modules of grammar may be far from trivial.

On the other hand, there is also no need to accept the conclusion that when it comes to the distribution of subject properties in individual languages, every dog is from a different village: that is, that the thirty-odd properties of (pre-systematic) subjects discussed by Keenan are in principle distributed completely independently of one another. In fact, these may cluster in coherent ways, since a number of properties may reflect a common organization within some domain, one of which ought to be the hierarchical organization of phrase markers and the operations that affect these, if syntax really has the character we think it has..

The methodological opposition here is the following: to judge from much functionalist writing, the pre-systematic categories of traditional grammar are widely taken to have a life of their own — there is a tendency to assume that they are unitary and immutable, and that our task is to identify them. Where we find that our theories do not provide a complete and categorical reconstruction of such a notion as "subject," we might conclude that it is actually fuzzy, prototype-like, etc. But why should we expect a notion like "subject," with all of the accretion of observations that it has acquired over the years, to correspond to a unitary theoretical category? Our goal in doing linguistics, after all, is to reconstruct not the

specific notions of traditional grammar, but rather the intuitions and insights that underlay them.

The alternative is to develop a theory that has some reasonably natural cleavages, such as "structural syntax" *vs.* "morphological form," "discourse rôle," "semantic rôle," etc. In each sub-area, we develop a coherent theory that reconstructs to the extent possible the insights of traditional grammar (sometimes finding that apparent generalizations were in fact illusory or epiphenomenal), as well as a theory of how principles from the various domains interact. The result is a view on which the syntactic ducks may or may not be morphological ducks as well, but in which we have a number of individually coherent senses of "duck *vs.* non-duck" rather than a single, monolithic but rather fuzzy notion of "duck," *tout court*.

The reason I have rehearsed these matters is not that I think it is important to defend a paper I wrote twenty years ago — one whose specific assumptions about syntax are probably indefensible within today's theories. Rather, it is because I think there is a rather general pattern of argumentation to be discerned in the functional/typological community's reaction to that paper and others like it, and an analysis of that reaction *is* important if one wants to understand how formalists respond to what they read in functionalist work.

The (functionalist) arguments in question are intended to establish the necessity of an appeal to extra-grammatical factors in analyzing grammatical structure — that is, the impossibility of the kind of autonomous analysis sought by most formalist syntacticians. Now when they look for the basic objects of investigation in linguistics, functionalists tend to see overall classes of constructions, often defined by semantic/pragmatic/discourse factors. But we have to ask what warrant we have for assuming that the descriptive taxonomy of previous generations of grammarians has succeeded in identifying unitary phenomena. How do we know that a notion like "subject" is conceptually homogeneous, or even that there is any sense to comparing "passive" constructions across languages, etc.?

What happens, however, is that some very broadly conceived term such as "subject," "passive," "agreement," etc. from traditional grammar, together with all of its pre-systematically associated baggage, is presented for analysis. When all of the related facts are considered, it is usually possible to show that some of them are intrinsically grounded in factors outside of the syntactic computational system. Therefore, it is suggested, an autonomous syntactic analysis of this phenomenon is impossible (or at best, misguided).

The problem with this argument is not with the supposed extra-grammatical factors themselves, but rather with the assumption that the theory ought necessarily to treat the term or construction in question as a single homogeneous object.

When we look more closely, it usually turns out that the single traditional term conceals an identification of phenomena from several areas of grammar, each of which individually has a perfectly coherent analysis within some appropriate subdiscipline in the study of language, but where no single component suffices to explicate everything. The formalist response to such an argument is that as, say, a syntactician, he or she is concerned with what is syntactic, but the rest is someone else's business, and there is nothing in the *syntactic* facts that have been presented that necessarily compromises the adequacy of a formal syntactic account.

I think that is what is going on in the case of the discussion of notions of "subject" in ergative *vs.* accusative languages: the strictly syntactic phenomena across languages conduce to a rather homogeneous and coherent account, though the connection between these and other phenomena that have been linked to the traditional term "subject" is much more varied. We can see the same logic in work that responds to Kuno's observations about reflexives by developing distinct theories of *local* binding, construed rather narrowly in terms of the syntax, and *non-local* binding, a rather different relation that is grounded in the formal structure of discourse representations.

Another instructive example is provided by an argument Croft (1995a:501) offers "against the self-containedness of syntax.":

> Babungo is a Grassfields Bantu language (Schaub 1985). Its noun class system is typical of Bantu languages, in that nouns fall into a range of noun classes, and the noun classes are quite arbitrary in their membership. That is, there is no way one could completely predict noun class membership on semantic principles, although Schaub notes some correlations, e.g. class 1/2 with humans (1985:174; the paired numbers refer to singular/plural forms). These agreement patterns indicate that noun classes are not only arbitrary but participate in a system of syntactic rules. Modifiers agree with their head nouns in class, and verbs agree with their core arguments in the same way. There is one exception to this generalization: anaphoric pronouns that refer to humans, regardless of the noun class of the human noun, take class 1/2 agreement (Schaub 1985:193). Anaphoric pronouns that refer to non-humans agree in the noun class of their nominal antecedent.

For Croft, the evident involvement of semantic factors as well as formal noun class in the choice of pronouns constitutes clear evidence that "the syntax of agreement in Babungo" cannot be analyzed in a self-contained way. But I think most formal syntacticians would deny that in Croft's intended sense there is any such

thing as “the syntax of agreement in Babungo” as a unitary phenomenon. On the one hand, there are agreement phenomena involving (on one formulation) some sort of SPEC-head feature checking or assignment within the clause, and this system is responsible for the agreement in noun class between modifiers and heads and between verbs and their argument. We have no reason to believe these are matters of anything but sentence form. On the other hand, there is the matter of the selection of an appropriate pronoun to refer to an antecedent that has been mentioned elsewhere in a discourse. This is surely not at all a matter of sentence syntax, however, and there is no reason to treat it as such, despite the fact that the noun class system which operates within sentences is one of the factors that contributes to pronoun choice.

Consider the following sentence:

(2) (My cat) Dent-de-lion is still hungry, even after it/she/he has finished all of its/her/his bowl of Skrunkies.

The choice of a pronoun here depends on people’s views about the sentience of their pets, knowledge of their secondary sexual characteristics, the existence of Nouns in other languages belonging to arbitrary gender classes, etc. Would anyone argue that these factors compromise the claim that subject/verb agreement in English is a formal process? There are, it is true, many unresolved puzzles in the syntactic analysis of English agreement, but the choice of pronouns for discourse referents is surely not among them, because it seems clear that entirely different components of linguistic knowledge are at work here. Indeed, a theory that *failed* to distinguish two quite distinct senses of agreement here could be said to be inadequate on that basis.

This point is made more concrete in a recent paper by Farkas and Zec (1995) which deals with facts in Roumanian that are entirely parallel to those of Babungo alluded to by Croft, and thereby shows how a formalist account of “agreement” might proceed. Farkas and Zec assume that the syntax involves rules of agreement in terms of a set of morphosyntactically relevant features (SG, PL, I, II, III, Masculine, Feminine) which are distributed by some process(es) of feature concord so that modifiers agree with their heads and verbs with their subjects. They also assume that an independent level of Discourse Representation Structure (along the lines of Kamp 1981 and Heim 1989) identifies discourse referents in terms of a distinct set of features (including [atomic], [group], [±Participant], [±Speaker], [male], [female]). A system of rules is developed that specifies the correspondence between the DRS properties associated with a referent and the grammatical features of the corresponding expression (including discourse-referential pro-

nouns). These rules accommodate puzzles with grammatical gender that have been pointed out by Corbett (1990), such as those motivating "resolution rules". The result is at least a substantial start on a full, formal reconstruction of agreement phenomena, involving the independent properties of at least two domains (morphosyntactic structure and discourse representation) and the systematicities in their inter-connections.

It is only by lumping together all of the heterogeneous phenomena that are associated with the notion of "agreement" that the Babungo facts appear to compromise the autonomy of syntactic analysis. But the goal of a formally serious analysis is precisely to disentangle the related but fundamentally independent strands of a superficially complex phenomenon. As a result, arguments in this style, which seem to reflect an essentially incomplete analytic understanding of the structure of the facts, have little direct force for the believer in formally serious, essentially modular analyses. This is the sort of discussion that David Perlmutter used to call "helicopter linguistics": you fly over the phenomena a few times, and assume that the view that you got from the heights represents the end of the story.

When we look at the functional/typological literature, unfortunately, this style of argument seems all too often to underlie the discussions that purport to show the necessity of including extra-grammatical factors in the analysis, or the non-categorial, fuzzy-edged nature of some grammatical term or category. This is certainly true for the discussion of grammatical relations such as "subject" or "object," and the same confusion extends directly to most functional typological discussions of ergativity. Ergativity, when examined closely, turns out to be a categorial parameter — or rather, several such parameters, primarily affecting the relation between syntactic structure and thematic or semantic structure, on the one hand, and morphological structure on the other. In any case, "ergativity" as a unitary phenomenon is a matter whose typological significance is unclear. What is generally referrred to in the typological literature (incorrectly, it appears) as 'Syntactic' ergativity is (roughly) a question of whether agents or patients typically appear as the subjects of transitive Verbs. Morphological ergativity is a property of particular rules, depending on which of two possible types of reference to syntactic structure is made by a given rule (see Anderson 1992 for some discussion of the mechanisms involved). There may or may not be a relevant parameter within the syntactic system *sensu stricto* (see Marantz 1996 for a survey of views on ergativity in recent formal syntax). Each of these notions is quite categorical, once we are clear about the proper locus within the grammar at which to raise the question.

Exactly similar remarks could be made about the claim of Hopper & Thompson 1980 that transitivity is a gradient, not a categorial notion. If you identify all of the

phenomena that cluster around verbal valence and argument structure as a single construct "Transitivity," it is not surprising that the reconstruction of transitivity turns out to be thoroughly heterogeneous — indeed, what would be amazing would be if things turned out any other way. But that in itself does not constitute an argument against the coherence of a straightforwardly syntactic, if rather less ambitious, concept of transitivity, namely the presence *vs.* absence of an object argument bearing a specifiable structural relation to a head. The other factors that commonly (but not universally) cluster around this structural notion have perfectly coherent analyses of their own that do not impugn the simplicity and generality of structural transitivity.

A standard rhetorical figure in the functional-typological literature is to identify a phenomenon, and then provide an explanation for it that crucially relies on appeals to extra-grammatical factors such as discourse topicality, metaphors grounded in the speech act situation, etc. Unfortunately, the objects of such explanations are sometimes epiphenomena, the result of spurious generalizations which collapse on detailed analysis. An example of this is the apparent correlation, in some "split ergative" systems, between case marking and verbal aspect: ergative marking is sometimes found in association with perfective verbal forms (or their reflexes, such as Hindi past tense forms) in a single language where accusative marking is used with imperfective verbal forms. The apparent connection between case systems and aspect has been something of a staple *explanandum* in typological discussion (see Delancey 1981, Croft 1995b, pp. 120f.).

In fact, however, as discussed in Anderson 1992 (pp. 354ff.), this apparent correlation is probably accidental. It happens that two common sources for the historical development of perfect aspect forms are (a) passives, and (b) possessive constructions, either of which can lead to a construction in which the (agentive) subject appears in an oblique case (which may be reanalyzed as an ergative). On the other hand, one source of innovative imperfective forms is an "object demotion" construction, which may result in the marking of objects with an oblique case that is re-interpreted as an accusative in an originally ergative language like common Kartvelian. These two developments are quite unrelated to one another, but happen to lead to similar outcomes in terms of the distribution of case marking across aspectually different forms. The details are complex, but once the individual cases are analyzed (rather than being lumped together), it appears that there is no more to explain in this case than in other cases of accidental convergence, such as lexical homophony.

Other such arguments resulting from the unwarranted assumption that descriptive similarity implies linguistically significant generalization, from incomplete

analysis or the gratuitous acceptance of traditional categories could be attested from the literature on "NP-identification phenomena," a notion which conflates the distribution of null pronominal subjects of infinitives, shared Noun Phrases in conjoined expressions, NP-pronoun coreference, identification of discourse referents, and many other areas of grammar.

The opposite of arguments from over-generalization is the argument from excessive particularization, which is also prominent in the functional typological literature. With respect to ergativity, it has become common now to point out that whole languages cannot meaningfully be characterized as "ergative" or "accusative," since we generally find some phenomena within a given language that could be regarded as organized on an ergative pattern, and others in the same language that are equally accusative. The response to this of work such as Croft 1990 is to suggest that such typological parameters apply not to languages but to individual rules. But this move goes too far in the other direction: in fact, the phenomena within any given language show some significant clustering relations, corresponding to the natural divisions of the grammar. The rules of inflection relating syntactic and morphological categories may indeed vary one from another in this way, but the rules of the syntax itself appear to be founded on a single, coherent notion of structure.

In this case, as in all of the others, the crucial step to an insightful analysis is to find the right granularity, the most appropriate decomposition of a complex phenomenon into individually coherent pieces. And of course, a central rôle in that process is played by the choice of an appropriate formalism, since it is really only after a potential analysis has been laid out formally that its components are really available for inspection. Ultimately, linguists of all stripes have to provide a formally explicit account of their analyses, as Croft (among some other functionalist writers) clearly recognizes. But despite this commitment, it is remarkable how rarely a fully explicit analysis of the grammar of relevant languages is provided to underpin functionalist discussion. Sometimes the absence of such an analysis is attributed to disagreements about what the "right" theory of grammar might be, as if such dissension obviated a detailed account. In fact, however, the longer a full explication is postponed, as it tends to be in functionalist accounts, and presystematic descriptive categories are allowed to stand in for the fully explicit account, the more likely it is that the discussion will go astray on the basis of suggestive correlations that do not ultimately correspond to primitives of linguistic structure.

5 Conclusions

The way to understand the overall grammar of a language, then, is not to assume that there is some unitary set of maximally inclusive and rather monolithic analytic categories, perhaps those of traditional grammar; nor yet to assume that there are no categories at all, only particular facts, and that all facts are equally contingent and mutually independent. It seems preferable to me, rather, to adopt a 'modular' point of view, and ask about the categories and types that are relevant to an understanding of what is orderly and coherent in each specific area of grammar. This entails, for example, analyzing the syntax on the basis of syntactic phenomena, while construing the morphology as providing hints but not unambiguous arguments; and then analyzing the processes of word formation in language and the ways in which syntactic information is used (or disregarded) in the operation of these processes; and then proceeding to comparable accounts of semantics, pragmatics, discourse structure, etc.

With respect to the study of linguistic typology, often thought to be peculiarly the province of functionalist interest, I do not personally believe there is any interesting distinction between "doing typology" and "doing theory." Typologists seek to find implications among linguistic phenomena, but this is not basically different from the theoretician's desire to elucidate the internal deductive structure of the theory. Typologists ought not to be content with merely observing that one grammatical phenomenon is correlated with another; but once they begin to ask what it is about the nature of Language that leads to the observed connections, they are looking for the same fundamental principles of grammar they would seek if they thought of themselves as "doing Theory."

Typologists do indeed pay great attention to the diversity of phenomena to be found in the world's languages, and not only to the complexity internal to individual languages; but the same can surely be said for any modern theoretician who wants a general theory. Indeed, much formalist research in syntactic theory today goes precisely by the name of Comparative Syntax, and seeks to delimit exactly the full range of variation found across languages with respect to syntactic forms. If a typology is an exhaustive characterization of this range of variation, it is just as much an object of desire for formalist grammarians as for functional typologists.

I am afraid I have gone on at rather great length with a somewhat arid discussion of methodology, rather than a spicy recitation of exotic facts and their explanation. But when one asks what formalists learn from functionalist work, the main part of the answer is likely to be: they learn that some further decomposition

is often warranted in factual domains which had traditionally been presumed to be logically unitary, or else that there may well be some deeper principle underlying a set of superficially diverse phenomena. No one has any reason to doubt that among the facts relevant to language and linguistic behavior in the broadest sense are many that are quite external to grammar. But while the functionalist response is to embrace these as evidence for the impossibility of an adequate formal syntax, the formalist asks where the natural joints within and among the facts lie, such that their full complexity can be seen to result from the interaction of individually simple systems based on broadly explanatory principles. Both of these reactions, of course, constitute research strategies rather than empirically falsifiable propositions about what the world is really like. But I think the modular line, with its assumption of individually autonomous systems, has proven sufficiently productive to be well worth pursuing, and that strategy rests essentially on the kind of full and complete explication that is provided by formalization. And that, perhaps, is what formalists learn from functionalists in syntax.

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